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ABSTRACT

This paper builds a strategy for ways in which instruction can be improved through research. The strategy is one that thinks of instructional changes as those that promise to make significant differences in instructional outcomes and then determines whether or not these changes fulfill their promises. The document cites evidence that supports the need for improvement in normal instruction, and then describes the author's strategy in three steps. The educational change strategist is enjoined to first define, classify, and schedule instructional objectives in terms of student performance; then analyze the performances to produce a specification of the demands they make on learners and to find analagous nonschool tasks that make these same demands (these tasks can also be used for research purposes); and finally formulate and test hypotheses that identify the principal sources of variance in attaining mastery of these tasks. The author then considers two hypotheses about the principal sources of variance in attaining mastery of tasks and speculates as to what instruction might be like if research were to produce evidence of their viability. (Author/DN)

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IMPROVING INSTRUCTION IN THE 1970s--

What Could Make a Significant Difference?

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In research, there are few guidelines for making strategy decisions. How do we decide what questions to ask? How do we decide which questions to ask first? How do we decide whether or not we have an answer? These are all complicated issues and we do not have routines for resolving them. But they are precisely the kinds of issues that are raised by the topic of improving instruction through research.

The purpose of this paper is to build a case for one kind of strategy. In brief, the strategy is to think of changes in instruction that promise to make significant differences in its outcomes and then to determine whether or not they do. The key word is "significant." As used here, it has nothing to do with statistics. Bluntly speaking, it means "large," on an absolute scale. Before concentrating on this strategy, however, it is necessary to deal with a prior issue, that is, the rationale for believing that instruction needs improving.

Signs That Instruction Needs Improving

An intent to improve instruction implies that there are some deficiencies in it. The ways we index these deficiencies are important since they will

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determine where we look for solutions. For example, if the principal deficiency of instruction is that gifted students do not realize their full academic potential, then the bulk of current instructional theory and practice can be left intact. Research effort can be concentrated on conditions that foster maximum growth in the gifted. In contrast, if instruction is deficient for a majority of the population, research should focus on more fundamental and comprehensive issues.

One index of instructional deficiency that might be generally recognized for its validity is rate of scholastic failure. A number of variables can be used as indicators of scholastic failure: for example, teacher grades, and standardized test scores. Unfortunately, neither of these is optimal for present purposes, because they provide a relative index of student performance and a relative yardstick is not the best kind for making an absolute judgment of the sort implied by the concept of failure. Nevertheless, we will adopt them here, mainly for lack of a better alternative.

Saddled with relative measures of student performance, it is important to find an entré to estimating failure rates. One way is to focus on group discrepancies in rate of scholastic failure. It may be possible to use these disparities for identifying instructional deficiencies. For example, it can be argued that the principal deficiency in public school instruction lies in its ineffectiveness for virtually all minority groups. Accordingly, it is worthwhile to examine the phenomenon of group inequality and its implications for identifying instructional deficiencies.

Group inequalities in scholastic success are easy to display. A common way is to select two groups such as white students from middle-class families and black students from poor families and to collect data on the grades

received and the standardized test scores obtained by students in the two groups. This method is illustrated in a study recently reported by McCandless, Roberts and Starnes (1972). Among the samples of seventh-grade children drawn from Atlanta Public Schools were a white middle-class group and a black-lower-class group. Average grades were computed for each sample in subjects like reading, arithmetic, language, science and social studies. Similarly, average grade-equivalent scores were computed from standardized test information in these subject matter areas.

The results were that the average grade obtained by white middle-class students was B minus while that obtained by black lower-class students was C. The two groups were also discrepant in terms of grade-equivalent scores on standardized tests with the black students from poor families trailing the white middle-class students by more than a "year" on the scale. These outcomes are fairly representative of the results wherever studies of this kind are conducted and they demonstrate that there are group inequalities in scholastic success.

In my view, such inequalities clearly signal the existence of instructional deficiency, but I do not think they accurately locate the source of the deficiencies. For example, group inequalities are compatible with the proposition that instructional deficiency is really a set of deficiencies in certain students that can be compensated for through special educational provisions. In practice, this proposition has materialized in the form of offering additional instruction to those who fail to profit from instruction normally offered. This strategy would be defensible if it worked and if normal instruction were successful for a vast majority of the population. Unfortunately, we have little evidence that compensation

works. Whether compensatory experiences take the form of additional pre-school instruction or the form of additional, remedial, instruction after formal school entry, they appear to have little enduring effect as judged by measures of subsequent scholastic performance (Parker & Tombari, 1972). I hurry to qualify this statement by recognizing that the issue is not closed and that the evidence is not conclusive. Nevertheless, the assessment I have offered is, at present, at least as well-supported as the alternative.

Even if the compensatory proposition could survive the test of effectiveness, it would founder badly on a second kind of test. Like any other set of special arrangements in society, compensatory programs make sense only if normal arrangements are effective for a very substantial majority of the population. It is questionable that normal instruction satisfies this requirement. If not, then it seems more sensible to alter normal instruction rather than to try somehow to compensate for its deficiencies. Thus, the possibility that normal instruction is largely ineffective deserves consideration.

School grades and standardized test scores can be used to examine the effectiveness of normal instruction so long as we keep in mind that both are essentially relative measures. Because of this, in order to use them at all, it is necessary to designate a cut-off point that defines failure on each of the scales, that is, school grades and test scores. One way of choosing such points is to imitate what we do in the case of group inequalities. When we compare the average performance of white middle-class and black lower-class students, for example, we conclude that the mean levels associated with the black, lower-class population indicate scholastic failure. Accordingly, let us define the cut-off point as either

grades or test scores lower than the average obtained by black students from lower-class homes.

Using this cut-off point and assuming that the data reported by McCandless et al. (1972) are representative of the country at large, estimates of scholastic failure can be calculated from information provided by the Bureau of Census. In 1969, the number of white children from middle-class families (annual income between seven and fifteen thousand dollars) was 32.4 million; the total number of black children from poor families (annual income below five thousand dollars) was 3.6 million. The failure rates in the two populations are, of course, discrepant: 33% for middle-class whites and 50% for lower-class blacks. When these rates are converted to absolute numbers, a total of some 12.6 million children in the United States are expected to meet scholastic failure. Of this total, middle-class white children constitute a staggering 10.8 million. Let me emphasize this point, the estimate is that more than 10 million, a full third of the population of middle-class white children can be expected to encounter scholastic failure. Thus, even if present group inequalities were fully diminished, a huge number of children would continue to suffer scholastic failure.

In order to believe that normal instruction is defensible, it is crucial to show that it is effective for that portion of the population usually regarded as well-prepared for school, namely, the white middle-class population. But, normal instruction appears inadequate for fully a third of this group. Adding this to the even larger proportions of other groups that fall below the cut-off point for scholastic success, normal instruction appears ineffective for between 40 and 50% of the total population. And, if a more stringent criterion were applied--mastery, for example--normal

instruction would have to be regarded as ineffective for an absolute majority of the national population.

In view of these estimates, a compensatory strategy for improving the effectiveness of instruction seems to be grossly inappropriate. Adopting such a strategy entails providing compensatory experiences for the bulk of the population. A plausible inference from the estimates is that the deficiency lies in normal instruction, not in the students it is supposed to serve. Accordingly, it is reasonable to work for changes in normal instruction itself with the goal of establishing schooling that assists all students in learning successfully.

Research for Instructional Change

The conclusion that normal instruction must be changed is only a starting point for discussion. Indeed, if normal instruction is as inadequate as the preceding examination suggests, it will not only have to change, it will have to change radically. To repeat myself, research must be designed to appraise changes that have the potential of making <u>significant</u>, large-scale, differences in the outcomes of instruction.

This kind of research task imposes special requirements on investigators. The most demanding of these is that of finding variables to manipulate that promise to produce truly massive effects. Let me offer a possible strategy that might facilitate the search for such effects.

The strategy is so simple that I am a little embarrassed to describe it. It consists of only three steps. The first is to define, classify, and schedule objectives of instruction in terms of student performance. The second step is to analyze the performances to produce a specification of the demands they make on learners and to find analagous non-school tasks that

make these same demands. These are the tasks that can be used for research purposes. The third step is to formulate and test hypotheses that identify the principal sources of variance in attaining mastery of these tasks. Once these steps are accomplished, it should be possible to make recommendations for instructional change, implement the recommendations, and evaluate their efficacy in terms of the objectives previously defined.

Hypotheses About Principal Sources of Variance

The third step, formulating hypotheses, is probably the most challenging. This step of the strategy is dictated by two considerations. One is the goal of locating instructional variations that promise to make significant differences in outcomes. The second consideration is our track record in educational research. The first consideration has already been discussed at some length. The second deserves at least brief explanation.

When I refer to our track record, I am not expressing dismay at the quality of our research and theorizing, even though these efforts deserve constant scrutiny and criticism. Instead, I am trying to convey my distress at the apparently marginal impact our research has had on educational practices and outcomes. Most of you probably share my frustration about this, some of you to the point of believing that we have enough research and theory in hand: what we lack are effective ways of insuring that it is disseminated and implemented. But, I want to take an alternative position--that our research has not focussed on variation that promises to make large differences in outcomes.

Reflect for a moment on the gross aspects of instruction. Between the time a child enters the first grade and completes the sixth, he has invested more than 5000 hours in school rooms. If he began school during the last ten

years, the child has had the benefit of new instructional materials and procedures, many of which--in reading and math, for example--have been hailed as revolutionary. Yet, after all of this variation in instructional conditions, techniques, and arrangements, and after 5000 hours of it, we are told that massive numbers of children leaving the sixth grade have not yet learned to read and compute adequately. Worse, as far as we know, these children have not learned to do anything well. It is staggering to believe that they have learned virtually nothing of what was intended for them after the magnitude of their investment, and ours, over that span of time.

It is not enough to counter this observation by asserting that materials and procedures have been improperly used, or that only a portion of the 5000 hours is actually devoted to instruction. If these assertions are true, than I fail to see how continuing our present course in educational research promises to result in any change.

These considerations, it seems to me, call for a change in the focus of our research: from attempts to improve current forms of instruction to a search for ways of utterly replacing present instructional patterns with new ones.

If so, the next question is where to look for changes that might make a difference. There are a number of possibilities, but let me draw attention to just two of them: age of the student, and the topic of learning.

Student Age. Think of virtually any intellectual task that we know about. With only a few exceptions, the data indicate that the older the student, the more efficiently and effectively he can learn to perform the task; moreover, the more likely that what he learns will be transferable.

This generalization seems to hold over an impressively broad range of tasks: associative learning; memorization; concept learning; and problem-solving, whether the tasks are from a Genevan mold or from an American mold. Thus, in my view, student age is a likely candidate for an instructional change that might make a difference.

The change implied by this suggestion, of course, is in the ages at which school performances are initially required. Bluntly, the imposition of these requirements should be markedly delayed. The hypothesis is that such delays would greatly facilitate the eventual acquisition of almost any skill or content we might designate.

I recognize that this hypothesis rests on a simple minded observation. I also recognize that it runs counter to an exceedingly strong tide in the direction of imposing requirements earlier in the child's life. And I recognize that the hypothesis frustrates the need to accelerate children intellectually. Still, I marvel that we have not yet made even a half-hearted test of it.

Learning Topic. Another hypothesis arises from an observation that is nearly as obvious as the one concerning age. Persons differ in the kinds of learning they can accomplish with facility and eagerness. Any of us could readily supply numerous anecdotes in support of this assertion but we can also call on even more convincing evidence. For example, so far, the search for a general learning ability has taught us that it just isn't there (Stevenson, Hale, Klein, & Miller, 1968). General ability seems to be reflected by instruments such as intelligence tests that index what has been learned previously, but not by instruments that directly measure a wide variety of learning. Even Piagetian theory could not survive without

the complicated notion of decalage (Piaget, 1972).

Let me emphasize that I am referring here to gross variations in topics and types of learning, not to variations of procedure within a single task. Thus, this hypothesis concerns the possibility that individuals differ widely in the ease and joy with which they accomplish different types of learning: reading vs. mathematics; science vs. social studies; observation vs. analysis; interpersonal skills vs. the skills of academic inquiry, and so on. If they do, the implication might be to alter drastically the requirements of scholastic success to provide a range of topical achievement broad enough to accomodate the range of individual variation in taste and facility.

I anticipate that this hypothesis will be seen by some as a recommendation in favor of watering down the academic content of schooling. This is not the intent. Instead, the point is that there is a startling variety of skills and contents that are worth learning, but normal instruction is directed to only a small fraction of these. Why not broaden the sweep of instruction. After all, as I have argued elsewhere (Rohwer, 1971) we have precious little evidence that doing well in school, as it is now constituted, is relevant for anything except doing well in school. Accordingly, there is little to risk and an enormous amount of possible gain in exploring the validity of the hypothesis that competence might be enhanced by instruction that offers, and honors, widely varying kinds of achievement.

Possible Changes in Instruction

Given these two hypotheses, let me conclude by speculating about what instruction might be like if research were to produce evidence of their

viability.

What would schooling be like if it were organized to capitalize on age and topical differences among children? The structure and procedures of schooling would need to be revised along several lines. First among the revisions would be an explicit confession that the aim of schooling is to insure student achievement at the end rather than at the beginning of the educational process. Even though high school education surely needs reform as much as elementary schooling, assume, for the time being, that the purpose of the early years is to prepare for success in high school; in the intermediate, or junior high school years, students would acquire the skills necessary to meet the demands of high school. In general, these demands would be for acquaintance with the conceptual structure, the methods of, and the information produced by a variety of disciplines--mathematics, literature, physical and biological science, psychology and so on. Obviously, students will make choices within this array as well as among various types of work necessary to acquire skills associated with clerical, mechanical and technical fields. Accordingly, the landmarks to be reached by the end of the intermediate school years--seventh, eighth, and ninth grades--consist of the prerequisites for these demands.

The prerequisites are relatively small in number. Chief among the skills in which instruction should be offered are reading and mathematics. The aim of reading instruction would be to assist students in learning to read rapidly and with comprehension. In other words, reading instruction would be designed to foster the skills of gleaning relevant information from printed material as efficiently as possible. In arithmetic, the aim would be to help students in acquiring the fundamental concepts and computational skills of elementary mathematics--very much the same material as is now required in the first six grades of elementary school. Instruction would

also be offered in a number of other skills. One of these would concern memorization and would include tactics for organizing material to increase its memorability. Assistance would be offered in the development of communication skills--interpreting and framing answers to questions, planning and preparing reports, methods of exploiting information sources such as libraries and people, lucid expression of ideas orally and in writing. Finally, instruction would be provided to foster thinking and problem solving skills, including methods for generating and evaluating ideas. Mastery of all of the skills mentioned would be mandatory for all students.

Besides work in the acquisition of various mandatory skills, instruction should be available in a number of elective skills as well. Typically, the elective skills would be more closely allied with particular disciplines than the mandatory skills. Students might practice various methods associated with scientific observation and experimentation, collect data on public opinion, learn interpersonal skills, acquire techniques of computer programming, attempt to teach younger children, conduct library research on topics of individual interest, analyze the structure, population, and sources of conflict in the local community, and so on.

Given this sketch of the curriculum for the intermediate years, what would schooling be like during the elementary years--the first six or seven grades of public school? This question can be answered by resolving the issue of what students need to be able to do in order to profit from the kind of schooling envisioned for the junior high school years. Here is where the proposed revision departs most obviously from the present structure of schooling. There would be no mandatory subjects--in the traditional sense--

for the elementary school years. Reading competence would not be required since reading instruction would be offered in junior high. Nor would arithmetic competence, nor competence in language arts, social studies, and science. As requirements, no one of these achievements would be required of all students. And no student would be expected to have achieved them all--or any single one of them, not even reading--by the time he enters the seventh grade. It would be the task of the intermediate school, not the elementary school, to look after formal mandatory instruction in these skills and disciplines.

Even so, there would be substantial work to be done in elementary school. This work would be relevant for the demands of junior high school; but, it would not supplant those demands by making them prematurely. The mandatory curriculum for elementary school would require that students select topics for study, do extended work on the topics, and succeed in the work. Success here refers especially to the quality of the work done and to completing the work planned. The topics available for choice should range widely and include all of those that will be available at later grade levels--reading, math, history, geography, sciences and so on. Sufficient latitude should be given so that a student could also select topics not usually included in public school curricula such as computer programming or the lore of professional baseball. Opportunities to acquire interpersonal skills should also be made available, especially in view of the enormous amount of the child's time that is normally devoted to dealing with his relationships with teachers and other students. In general, the elementary school curriculum would be designed to afford children repeated experiences of what work is like and what success in school work is like.

In an elementary school of the kind envisioned, the teacher would have three major functions: offering guidance in the selection of topics; providing encouragement and resources necessary for the child to pursue his work on a topic; and giving suggestions and support to aid the child in improving his work on the topic. With regard to the selection of topics, the guidance function would be extremely important. It would be the teacher's task to determine whether or not a child has a reasonable chance of succeeding in his work on each of the topics chosen. For example, if a child wished to learn to read, the teacher would need to determine that the child could do so without so much difficulty that it would defeat the purpose of the elementary years.

The task of providing encouragement would not be an onerous one, but that of making resources available could be quite demanding. Here the teacher would have to be supplied with resources by those who develop curriculum materials. Such materials would have to meet two principal standards: that they could be used by the elementary school child without substantial adult guidance; and that their use not require skill in an unrelated subject. For example, curriculum materials to be used in work on scientific topics should not require reading skill as a prerequisite. That this standard can be met has already been demonstrated in the elementary school science curriculum developed under the auspices of the American Association for the Advancement of Science.

Lastly, the function of giving assistance to the child in improving his work would replace the present practices of grading a child's work and of using standardized tests of achievement to determine his progress. Clearly, under the system of schooling envisioned such methods of evaluating

students would be inapplicable. The work of elementary school students would be so diverse that it would not be feasible to construct standardized tests. Instead, the method of suggestions leading to improvement would both encourage and demand success of the child in producing completed work of high quality.

A two-part rationale can be given for revising schooling in the manner proposed. First, there is reason to hope that the type of elementary schooling envisioned here will significantly diminish, if not completely remove, the experience of academic failure in the lives of young children. This should allow virtually all children to embark on the intermediate years of formal instruction with feelings of genuine liking for learning rather than the fear and hostility that so many children bring to those years at present.

The second part of the rationale rests on a dual proposition: that all of the learning necessary for success in meeting high school demands can be accomplished in only two or three years—the junior high school years; and that delaying the beginning of prescribed instruction until those years holds promise for many children of increasing the ultimate degree of academic success they can achieve (cf. Humphreys, 1971; Elkind, 1969; Rohwer, 1971).

So far, there is no real proof that the system of schooling I have sketched would solve the problems of instructional deficiencies. But advance proof is not the issue. In fact, the only way to obtain proof is to do the necessary research. The real issue is whether or not the rationale for the system is plausible enough to warrant a test. Overall, I think it is.

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